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Interactive comment on "Impact of peatlands on carbon dioxide (CO_2) emissions from the Rajang River and Estuary, Malaysia" *by* Denise Müller-Dum et al.

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Thank you for taking the time to review our manuscript and for your comments and suggestions. We believe that we can satisfactorily address each of your comments.

General comment: The manuscript focus on an important topic that I believe is suitable for publication in Biogeosciences. The transport and emission of carbon/GHGs from river networks has repeatedly been concluded during the last decade as a highly significant component when for example estimating landscape C budgets at various scales and biomes. Although the importance is well-recognized, I would claim that relatively little is known about large rivers and their source contribution of atmospheric

CO2. The knowledge that exists is largely restricted by the spatiotemporal resolution of the measurements or by using data being based on indirect measurements of pCO2. There is also a clear bias in existing data-sets towards northern hemisphere river networks and with limited information of tropical rivers, especially south-east Asian ones. In this context this study aims to fill an important gap in our understanding concerning large scale drivers of aquatic C in river networks. The influence of peat deposits in the catchment on the pGHG in the water has been shown for various biomes and river network sizes but more extensive investigations are needed. Hence, this is a highly relevant topic especially for a tropical region like this.

Although the aim of manuscript is important I have some concerns on how suitable the manuscript is for publication in its current form. My main concerns are: 1) How the actual emissions are calculated. I understand that this is a data scarce region but the way the authors have estimated the emissions is not especially convincing. The author's measure pCO2 in a satisfactory way but the entire k calculation component feels very shaky. No actual measurements of any of the input parameters are conducted. A vague estimate of a fixed water velocity is used in combination with modelled wind data. Three different k parameterizations are then used gaining slightly, to very, different outputs. The model producing intermediate k estimates are then used without any stronger further motivation. The whole procedure feels as I already said very shaky, without knowing anything about the river, investigating seasonal differences in emissions and then using a fixed water velocity sounds for example very strange. On top of these vague calculation steps there are no uncertainty estimate of the calculated emissions (or lateral exports of inorganic and organic C!!). To describe and estimate this in a transparent way would be a requirement in my eyes, especially due to the scarcity in data for the k calculations. If this is problematic to handle, one suggestion is to skip the emission data and solely present the pCO2 patterns and how it varies with wet and dry season and the influence of peatlands. Personally I think this would be the way to go and would be highly interesting in itself. 2) I am not totally *convinced of the interpretations of the 13C-DIC data, I am surprised by the generally high 13C-DIC values, the authors claim that the contribution by carbonate containing bedrock to the riverine DIC is minimal in the area and that the river is affected by tidal water sustaining the estuary with marine DIC. That is likely correct but the high 13C-DIC is found even in upstream non-peat area, is the evasion the sole explanation for that? Maybe not relevant, but what about methane production, I understand that methane might have been included in the original plan, but if methane in the peatlands is mainly produced by CO2 reduction this will heavily influence the 13C of the CO2 being delivered to the river (See Campeau et al. 2018 for example). Overall, I find the interpretation of the 13C-DIC data quite short and not as well developed as it could be. 3) Is it really correct to talk about seasonality when just two measurement campaigns are conducted, i.e. wet and dry season? I am not familiar with the region but to call something seasonality or similar would in my mind require a higher sampling resolution in time.*

Thank you for highlighting the relevance of our research and for suggesting improvements. We would like to respond to each of your main concerns first. Below, we will respond to each of your minor comments.

1) We agree that k is the most uncertain parameter in CO_2 emission calculations. It is very common in the scientific literature to use one of the available k-parameterizations to calculate CO_2 emissions, and only in very few cases were authors able to provide both pCO_2 measurements and flux measurements at the same time. Of course we understand that just because something is usually done in a certain way, it doesn't mean it is also justified. We agree that the paper would also work without the CO_2 emission calculations and in principle, we are open to this modification. However, rather, we would keep the CO_2 emission estimates as part of our study and we have two main arguments for that:

The first one concerns the insights that a CO_2 emission estimate allows. We agree that it is necessary to better account for uncertainties, but keeping CO_2 emission estimates as part of our analysis allows us to set lateral transport in relation to CO_2 outgassing. This is important in the light of the "active pipe" hypothesis, which sees rivers as active conduits and locations of carbon processing and outgassing instead of mere transport pipes. We can only make a contribution to the validation or falsification of this hypothesis if we provide estimates for both lateral transport and vertical outgassing. Therefore, we would like to keep CO_2 emission estimates as part of our analysis.
However, we agree that uncertainties must be accounted for in a more transparent and suitable way. We think that singling out one of the k-parameterizations as the preferred one might be the main reason the whole procedure feels shaky. While we think that we do have some justification for that (Borges et al. 2004 were the only ones who considered flow velocity as driver of turbulence), we acknowledge that the uncertainty in k must be better accounted for. Therefore, we will present the different parameterizations that we found suitable for our river as equally justified and interpret the range of values that they yield as a range of uncertainty introduced by the decision to use a parameterization for k. In summary, this means: We will present all three parameterizations by reporting an average, minimum and maximum estimate. This way, it is easier for the readers to get an idea of the uncertainty.
The second argument is related to the reception of scientific evidence by the readership. There is, to our knowledge, only one other CO_2 emission estimate for the Rajang River (Chen et al., 2013). Those authors used a k-parameterization to calculate fluxes (Wanninkhof 1992) and pCO_2 data from one season (inter-monsoonal) only. Our data increase the data density and considering the importance of CO_2 emissions we find it important that all available data is at hand and accessible for the scientific community. Accessibility is a lot easier when we report data in matching ways, therefore, it would be important for us to provide a CO_2 flux estimate that can be compared to existing ones.

2) Our measurements only cover the lower river reaches (approx.. the last 200 km), so outgassing might indeed be a valid explanation for the high $\delta 13$C-DIC. Before the river reaches Kapit (which is the point up to which we have measurements), it flows through mountainous terrain, even including rapids, where high outgassing might occur. However, we will expand and further develop the discussion of $\delta 13$C-DIC in the revised manuscript, including a Keeling plot that should support the discussion of possible sources and also including processes that we had not mentioned so far, like methanogenesis.

3) A higher sampling frequency also during inter-monsoonal periods would certainly be desirable, just like interannual sampling. Our data from the peak of the wet and dry period is, of course, only a snapshot. The terminology "wet and dry season" still seems appropriate to us, as it describes accurately when the samples were taken. However, we agree that it is not possible to make strong claims about seasonality using this data. In the revised manuscript, we will make it clearer that "wet and dry season" is mainly a terminology and that our data are too few to make strong claims about seasonality. In Section 4.2.2, we would add a sentence: "As our data was collected during two single surveys, they represent only a snapshot and do not allow strong claims about seasonality."

Detailed comments:
P3 Ln 1-10, there is a mix of wetland and peatland, consistency or a clear separation would be good.
Agreed, Borges et al. (2015) consider different kinds of wetlands in their analysis, while the Wit et al. (2015) study focuses on a specific kind of wetland (peatland). We would try to rephrase and suggest the following change: "Two regional studies independently showed that the partial pressure of CO_2 (pCO_2) in rivers increases with increasing wetland coverage in the catchment. Borges et al. (2015) established a relationship

between wetland extent and pCO_2 for African rivers. Wit et al. (2015) presented an analog synthesis for Southeast Asian rivers, which flow through peatlands. Peatlands are a special type of wetland, where organic matter accumulates at rates that make them the most effective terrestrial carbon store on a millennial timescale (Dommain et al., 2011). Southeast Asian peatlands store 68.5 Gt carbon (Page et al., 2011). The highest riverine dissolved organic carbon (DOC) concentrations reported so far were found in Southeast Asian peat-draining rivers (Alkhatib et al. 2007; Moore et al., 2011; Müller et al., 2015), with an annual average of 68 mg L-1 DOC found in an undisturbed peat-draining river (Moore et al., 2013). Because of these high DOC concentrations, Indonesian rivers may account for 75 % of the DOC flux into the South China Sea (SCS) while accounting for 39 % of the discharge (Huang et al., 2017). Surprisingly, CO_2 emissions from these rivers are not exceptionally high (Müller et al., 2015; Wit et al., 2015). This is attributed to a short residence time of the organic matter in the river, allowing little time for decomposition, and the resistance of peat-derived carbon to bacterial degradation. Nevertheless, the CO_2 flux from peat-draining rivers to the atmosphere increases with increasing peat coverage in the river basin (Wit et al., 2015), showing that these ecosystems exert an important influence on a river's carbon budget."

P3 Ln 11, Odd formulation and scientifically a bit weird. To claim that something is the highest worldwide is only true until someone else present a higher number. I would recommend to be more open in this formulation.
We would rephrase: "The highest riverine dissolved organic carbon (DOC) concentrations reported so far were found in Southeast Asian peat-draining rivers (Alkhatib et al. 2007; Moore et al., 2011; Müller et al., 2015), with an annual average of 68 mg L-1 DOC found in an undisturbed peat-draining river (Moore et al., 2013)."

P5 Ln 20-25 and 30, what about correction for salinity on the pCO2 and emissions?
Correction to pCO_2 was not applied, because salinity impacts the solubility of the gas, not its partial pressure, which is a notional variable. The independence of partial pres-

sure from salinity or water temperature is an asset when comparing CO_2 in rivers across seasons or from different locations. In contrast, CO_2 emissions to the atmosphere are dependent on salinity, because the actual concentration of a gas is used in the calculation of its flux to the atmosphere. This is accounted for in our calculations in the calculation of CO_2 solubility according to Weiss (1974) and the calculation of the Schmidt number according to Wanninkhof (1992), which is used to adapt diffusivity and thus the gas exchange velocity k to different salinity and temperature. The information how we calculated $k_{T,S}$ from k_{600} was indeed missing in the current version of the manuscript and will be added to the revised paper.

P6 Ln 8-10 Isn't water velocity dependent on discharge, why is a fixed value used???
In our response, we are using:
Q = Discharge
w = Water velocity
k = gas transfer velocity
It is correct that w depends on Q. Raymond et al. (2012) found that w scales with $Q^{(0.29\pm0.01)}$. In the absence of w measurements during our study, we had to resort to a literature value. We felt like it made more sense to use a literature value from the same river instead of calculating w with an empirical equation that was developed for rivers in the United States. However, from the hydraulic equation of Raymond et al. (2012), we can still get an idea about how variable w might be. Our reasoning is as follows: Ling et al. (2017) report w = 1.1 m/s. From the description of their work we infer that this is an average of all measurements they carried out in August 2014 and January 2015. The measurements of Staub and Esterle (1993) were carried out in July and August 1992, and a range and the average value are given in their paper (w=0.7 m/s). As both those estimates seem equally valid, we did not single out one but used their average of w = 0.9 m/s as a general average flow velocity in the Rajang River. During the monsoon season in January, Q increases by 50% compared to the average value. According to the equation by Raymond et al. (2012), a 50% increase in Q would result in a 12% increase of w. If we assume this variability, w in the Rajang River would vary

with Q from 0.8 to 1.0 m/s or 0.9 $\pm$ 0.1 m/s. This would add an uncertainty of 4% to the Borges et al. (2004) k-value. However, the uncertainty introduced by the use of different k-models (A11, R01, B04) is much larger: A11 yields up to 30% higher fluxes than B04 (the intermediate), and R01 resulted in up to 70% lower values. This shows that the choice of model is the biggest source of uncertainty. Therefore, we suggest that we include the described error analysis in the Supplement, but stick to our plan of deriving the overall uncertainty of k from the presentation of the different models.

P6 Ln 10, Is there no wind data to validate this modeled data with? How accurate is the wind data compared to conditions over the river is tricky to judge. Feels very vague and uncertain!!!
Unfortunately, we were unable to obtain wind speed data measured on site. So the NOAA NCEP Reanalysis data was the best available option. This is a solution authors resort to if no on-site wind speed data is available (e.g., Bouillon et al., 2012; a number of Russian estuaries reported in Chen et al., 2013). We will point out the uncertainty introduced by the choice of wind data in the discussion of the CO_2 emission estimates.

Also, how was water depth measured, it is not mentioned as far as I see, but included in the B04.
That is correct, this information is missing. Depth was recorded at each station from the bottom sounder of the boat. This information will be added to the revised manuscript.

Based on the fixed water velocity and fixed wind?? Is a constant k used for each season independent of location along the river?
Yes, since we derived wind speed for an entire grid and used a literature value for the water flow velocity, we had no choice but to use one k for the entire river for each season. We agree that this is not 100% satisfactory and will include some more justification in the Methods section. We also think that using the new approach following your main comment, uncertainties might be better accounted for.

P7 Ln 29-30, a bit odd that POC was measured but not DOC. Hard to redo the study

but how relevant are the literature DOC values for this study, please motivate better!
In fact, DOC samples were taken but the values had to be discarded because contamination was suspected. The DOC measurements of Martin et al. (2018) were taken during three campaigns in 2017 and covered the Rajang delta downstream of Kanowit. The author provided us with the DOC values that he published in his 2018 study and we chose only those that were taken at zero salinity. We believe that these DOC values are the best available estimate for the Rajang River.

P8 Ln 25, please clarify what pH that is for wet resp. dry season.
This will be done in the revised manuscript.

P9 Ln 10-12, was not the purpose to investigate if the peatlands have an influence on the pCO2 in the river. Feels a bit strange then to say that too few 13C-DIC samples were taken.
Yes, the purpose was to investigate the impact of peatlands on pCO_2. After the first survey and looking at the data, we found that the measurement of additional parameters might be helpful, so DIC and $\delta 13$C-DIC were measured during the second campaign. However, resources were limited so the number of samples is not sufficient to make a statement about statistical significance.

P9 Ln 20, here and elsewhere, what is "distributaries", isn't just tributaries enough???
To our understanding, tributaries are rivers that flow into the main river. “Distributary”, in contrast, is a word that describes when a river branches off from the main river. Thus, “tributary” and “distributary” describe two different things. In our study, we use the term “distributary” because the Rajang River does not discharge through one river mouth, but splits up into several “arms” (or “distributaries”) before discharging into the sea. We use the term “distributary” in accordance with other descriptions of the Rajang River system by Staub and Esterle (1993), Staub et al. (2000), Staub and Gastaldo (2003).

P9 Ln 21-23, important sentence but feels more like discussion than result!!

Agreed. The first half of the sentence is a result and will stay here, the second part of the sentence belongs to the discussion and will be moved to the right section.

P9 Ln 27-28, again, feels more like discussion to me.
We would like to keep this statement about night time measurements of CO_2/O_2 here in the Results section. We inserted it after stating a correlation between CO_2 and O_2. An immediate question a reader might have is, if CO_2 and O_2 co-vary, is that due to diurnal variability? Therefore, we would like to take this thought up and quickly clarify that we are unable to make a statement about diurnal variability. We are not providing any further discussion, just stating the fact that not enough data was available, which we feel is appropriately placed in the Results section.

P10 L4, what does the +-0.52 and +-0.45 mean? Some kind of uncertainty or just spread? Please clarify in the methods. The emission rates (and lateral exports of C) are hard to get a feeling of, how uncertain are they? Impossible to judge for the moment.
This is the spread of the data and comes from the spread of pCO_2. However, following your main comment (1), we will now report CO_2 fluxes differently as described above. The reported errors will be described in the Methods section 2.3.

P10 Ln 17-20, Feels from a reader perspective a bit odd to start to say that the findings are the same as found in other studies. I think the authors could “sell” their study better than that. It is important information but I would not place it first in the discussion.
We will rewrite this paragraph and not put those 3 lines first in the discussion. However, we would still like to start the discussion off by generally characterizing the Rajang River (see next comment) and placing it in the “bigger picture” before we start the detailed discussion of pCO_2.

Also, maybe a matter of personal taste, but why not start with the main focus of the manuscript in the discussion (pCO2 patterns and maybe emissions if included), the SPM and POC story is secondary as I see it.

We agree that the SPM and POC story is secondary. We had the choice between reporting and discussing SPM/POC first, last, or leaving it out completely. The last option seems inappropriate, as SPM and POC measurements were conducted, the data has good quality and might be interesting to many readers. We saw no reason to exclude it. About the position in the manuscript: We thought that if we lay out the pCO_2 discussion first and then add SPM/POC, the whole SPM/POC discussion might come across as an afterthought. In the end, we decided to provide the reader with a general characterization of the rather unfamiliar Rajang River before detailing our thoughts about pCO_2. This general characterization also includes the SPM and POC data. We agree that this might be a matter of personal taste, but we feel that the SPM and POC story should be part of the general characterization followed by the more detailed discussion of pCO_2. Therefore, we would like to keep the current order.

P12 Ln 11-14, Likely true but there is also a strong fractionation in 13C-DIC related to changes/differences in pH which could be up to ca 10 per mille.
As stated in our response to your three major concerns, we will expand and deepen the discussion of $\delta 13$C-DIC and we thank you for providing another highly relevant reference that we had not considered so far. As for pH, we are reporting the isotopic signature of the entire DIC pool. For a fixed DIC, a change in pH would certainly influence the equilibrium fractionation within the carbonate pool, but it would not change the $\delta 13$C-DIC. Of course, pH would influence $\delta 13$C-DIC if DIC is added or removed from the system (as, e.g., in CO_2 evasion). We will point out the influence of pH in the expanded discussion of $\delta 13$C-DIC.

Table 2.What is the +- of the emissions, the SE of the mean? I.e. some kind of measure of the spatial variability? Is this driven by something else than just variability in pCO2? Is k fixed for all data? According to the methods I get this feeling. Please clarify in the methods.
Yes, the $\pm$ is the SE of the mean. This is stated in the caption (mean $\pm$ SE). The abbreviation "SE" is introduced in the caption of Table 1. As we present averages from

our longitudinal surveys, the SE is a measure of the spatial variability. For pCO_2 and FCO_2, it is driven by the variability of pCO_2. For O_2 and DIC, it is driven by the spatial variability of these parameters, respectively. As described above, for k one value for the wet and dry season was used, but we intend to provide a range instead in the revised manuscript. We would clarify in the Methods section and provide the range of k and FCO_2 values in the results table to make it clearer.

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